

Interactive comment on “Quantifying Vertical Hyporheic Exchange and hyporheic residence time in thalweg paths of meandering streams characterized by multiple riffle-pool sequences morphology” by Aminreza Meghdadi et al.

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Dear authors,

Because one reviewer missed the deadline and did not react to any of my e-mails I decided to review the manuscripts myself. Since I am also editor of this manuscript, I would like to stress that I did not read the first review when doing my own. So my decision as editor (taking the other review into account as well) may differ from that as reviewer.

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The manuscript describes different methods to quantify the hyporheic exchange fluxes and residence times on several locations along a river characterised by pool-riffle sequences. The different methods were 1) using vertical temperature profiles; 2) using vertical radon profiles; 3) using vertical EC profiles and 5) by calibrating a hydrological model on groundwater heads.

While the results of these different methods has the potential to gain insights on the pros and cons of the different methods as well as insight in how such systems work, the novelty of this work is not very clear to me. The four objectives are rather detailed and more at the level of sub-questions, while an overarching objective was missing.

Furthermore, the structure of the manuscript could be significantly improved. There is little structure, especially in the results and discussion section, without much synthesising of the results from the different methods. Among others: my advice would be to separate the results from the discussion section. Currently, it is often unclear what the finding of this manuscript are and what findings of others are.

Altogether, I find these shortcomings too much to warrant major revisions, but I encourage a resubmission as a new manuscript (if the authors can answer all raised questions satisfactory – especially the novelty of the manuscript should become clear). More detailed comments are given below:

P4 L17: this paragraph is merely namedropping. Indicate limitations/made assumptions etc. that makes the novelty/niche of this study clear.

P5 L4: This paragraph contains too much detail of the model. This should go to the methods section.

P5 L20: which of these aims is the main objective?

P9 Eq. 3: Why not simply stating that VHE equals q ? You can make this statement already when describing Eq. 1

P10, section 3.2: More information should be given here: Describe that this test is

done with a slug test, where the sediment is inside the tube. Also refer to the original literature in which this method has been described (i.e Chen 2000 - Environmental Geology)

P10 L15: So K_v was about 8 times higher than ID ? I guess it should be the other way around. Also explain why this is important: is it to say that the resistance of the tube is much lower than that of the soil?

P12 Eq. 6: a small sketch of all the fluxes in and out of the grid cell would be useful. Is there also lateral flow of groundwater and or river water?

P13 L12-13: What do you mean with this? What do you mean with flux momentum: density times velocity? Also add a unit after '0.5' (do this after all numbers throughout the document!)

P13 L17-18: This is a clear example of how your own results are mixed with those from literature. To me it is unclear what your own findings are and what comes from literature. By separating the results from the discussion, this can easily be avoided.

P14 L1-2: where do these results come from?

P14 L6: How are these K_v values derived?

P15 L1-2: This is another example where it is unclear if these are your own findings or findings from others

P15 L15: The reference to Kasahara and Wondzell (2003) seems a bit odd here, since you are talking about your own results.

P16 L5-6: the water fluxes are by definition driven by a difference in hydraulic head. The question is more why at one point the groundwater head is higher than the river head while at other points it is the other way around.

P16 L12: This implies that the temperature derived ones are not accurate.

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P17 L5: how do you know that VHE_T is an overestimation? Maybe radon underestimated it

P19 L15: a ratio of one means that the error is as big as the head difference, right? That sounds like a large error to me.

P20 L14: How large is the RMSE and MAE in relation to the VHD? (also add units after the numbers)

P22 L1-2: Could it be that direct heating of the streambed by solar radiation plays a role here? This would mean that more heat is being transported in downward direction, leading to a lower t_r

P22 L3-5: Why would this argument not be valid for tr_{Rn} ?

P22 L12-15: Why not the other way around? The influence by adjacent fluxes introduces, in my opinion, an error

Table 1: Within each zone more than one sample has been taken, right? The result of which sample is shown here? I suspect it is the average of all the samples in one zone. However, that does not say anything given the large heterogeneity within each zone

Fig. 1:

- indicate the insets in the middle figure with squares instead of circles (the same area of the zoomed displays)
- The North arrow seems to be wrong given the square drawn in the top right map
- The sampling points are hardly visible
- It seems from this figure that the installation given in panel B has only been installed at sampling point B7, but I understood that it has been installed at all sampling point. Is that correct

Fig. 2: add a scale bar

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Fig. 4: It would be easier to interpret this figure when it is a 2D figure

References:

Chen, X. Environmental Geology (2000) 39: 1317.
<https://doi.org/10.1007/s002540000172>

Kasahara, T., and Wondzell, S. M.: Geomorphic controls on hyporheic exchange flow in mountain streams. Water Resources Research, 39(1), SBH 3-1. doi:10.1029/2002WR001386

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2019-446>, 2019.

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